

DOCUMENT RESUME

ED 138 102

FL 008 522

AUTHOR Kay, Paul
TITLE Some Theoretical Implications of Ethnographic Semantics. Working Paper of the Language Behavior Research Laboratory, No. 24.
INSTITUTION California Univ., Berkeley. Language and Behavior Research Lab.
SPONS AGENCY California Univ., Berkeley. Inst. of International Studies.; National Inst. of Mental Health (DHEW), Rockville, Md.
PUB DATE Oct 69
GRANT MH-25703
NOTE 24p.; Paper presented at the Annual Meeting of the American Anthropological Association (68th, New Orleans, Louisiana, November 23, 1969)
EDRS PRICE MF-\$0.83 HC-\$1.67 Plus Postage.
DESCRIPTORS *Anthropology; *Componential Analysis; Distinctive Features; Language Research; Linguistics; Psychology; Scientific Methodology; *Semantics; Sociocultural Patterns; Taxonomy; *Vocabulary
IDENTIFIERS Anthropological Linguistics; *Ethnographic Semantics

ABSTRACT

Ethnographic semantics is that discipline which seeks to understand human cognition through an analysis of the cognitive content of linguistic expressions: that is, the systematic study of the meanings of words and the role of these meanings in cognitive systems. There are many misconceptions about the nature of ethnosemantics, however, and by exploring these misconceptions and by using the ethnosemantic method of contrast to establish what ethnosemantics is not, it is hoped that the nature of the discipline will be made clearer. Evidence is presented to refute six basic misconceptions about ethnosemantics: (1) it takes native generalizations at face value, (2) it is concerned with methodology for its own sake, (3) it is anti-theoretical and anti-comparative, (4) it is limited to domains in which there are external classifications, its methods being inapplicable to domains not involving genealogical relationships, (5) it is anti-evolutionary and does not give objective accounts of cultural systems, and (6) it is anti-statistical and unconcerned with prediction. (CLK)

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SOME THEORETICAL IMPLICATIONS
OF ETHNOGRAPHIC SEMANTICS*

Paul Kay
University of California, Berkeley

U.S. DEPARTMENT OF HEALTH,
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Working Paper No. 24
Language-Behavior Research Laboratory
October 1969

The Language-Behavior Research Laboratory
is funded by a grant from the Institute of International Studies,
University of California, Berkeley,
whose support is gratefully acknowledged.

*Paper read at the annual meeting of the American Anthropological Association, November 23, 1969, New Orleans, Louisiana.

FL008522

The familiar material objects may not be all
that is real, but they are admirable examples.

W. V. O. Quine (1969:3)

I think Quine's caution to the philosopher in search of the nature of
ultimate reality can be adapted for the anthropologist in search of the nature
of culture.

The informant's most careful statements about the
nature of his world may not be all the ethnographic
data, but they are admirable examples.

Neither statement is exclusivist. Quine does not limit the real things to
familiar material objects; nor does the "new ethnographer" so-called limit
his data to the verbal formulations of his informants. * Practitioners of the

* The "new ethnography" as Marvin Harris accurately points out is not all
that new. Rather it is a recent development, which adds a measure of
methodological and theoretical precision, to a venerated anthropological
tradition stretching back through Kluckhohn, Malinowski, and Lowie to
Durkheim and even Dilthey--a tradition which, it appears, Harris feels is
~~more~~^{better} honored in the breach than the observance (Harris 1968:597-598).
Nevertheless, I will use the incompletely felicitous term "new ethnography"
rather than add yet another term to an already overcrowded lexical domain.

new ethnography do many things besides ethnosemantics, depending on their
problems and their tastes. They may take censuses, count yams, take ~~series~~^{aerial}
photographs, learn to identify several thousand plant species, and so on.
They also just sit around and watch and listen and empath~~ize~~^{size}, as all
ethnographers do. But whatever else they may do, they always do ethno-
semantics. That is, the new ethnography seeks through interview, observation,
and when possible experiment, to discover some part of the system of meanings
by which people organize the world. The goal is the raw cognition if you will, but

since the major realization of this cognition is in the words people speak, semantics is considered an integral part of ethnography. *

* It has long been claimed in anthropology, and correctly so, that not all culturally important categories are directly reflected in the lexicon. Significantly, the actual demonstration of this claim awaited the work of ethno-semanticists, especially Berlin and Black (Berlin, Breedlove, and Raven, 1968; Black, 1969). The significance of this finding is ^{first that} most of the culturally important cognitive categories are represented directly in lexicon and, secondly, that the best way to arrive at the linguistically covert categories is to use as input for that search the output of a semantic analysis of the lexically encoded concepts.

The task of establishing some sector of the culture bearer's tacit theory of the world is a necessary part of the new ethnography. Many specialized techniques are used in the attempt, and new ones are constantly developed. This is not the time or place to present a catalogue of those methods. Rather our business here is to discuss the findings of ethnoscience rather than its methods, and particularly the theoretical findings and general theoretical position of the field.

Above and in what follows, I use the expressions ethnographic semantics, ethnosemantics, and ethnoscience synonymously and interchangeably to refer to the systematic study of the meanings of words and the role of these meanings in cognitive systems. I would contrast ethnography, new or old. The new ethnography (or linguistic or cognitive ethnography) is that approach to ethnography which includes ethnosemantic study as a necessary part. Practitioners of the new ethnography employ many methods other than ethnosemantic ones, in particular all the traditional techniques of anthropological field work, including participant observation; they also use a host of specialized techniques ranging from soil analysis to multi-dimensional scaling.

I have argued that ethnosemantics does not exhaust the content of the new ethnography. It is equally true that the theoretical relevance of ethnosemantics is not limited to the field of ethnography or even to anthropology. Ethnographic semantics is that discipline which seeks to understand human cognition through analysis of the cognitive content of linguistic expressions. It is therefore, in principle at least, as much a part of linguistics and psychology as of anthropology. Should any anthropologist be inclined to interpret the last statement as an admission of guilt, I would suggest to him that anthropology is unlikely to profit from a self-imposed jurisdictional isolationism. I hope we can all agree that science is advanced when we define scientific disciplines in terms of natural groupings of phenomena, rather than the other way around. The system of meanings--the tacit theory of the world--lying behind a language and its usage is a natural phenomenon worthy of scientific study. Ethnographic semantics is that study. The results of ethnographic semantics are variously relevant to ethnography, cultural anthropology, linguistics, and psychology.

One of the major methodological principles of ethnoscience is the method of contrast. One establishes the meaning of a word (that is, the concept realized by a lexical item) by exploring the features that cause it to contrast in usage with any word that can be substituted for it. One discovers what the concept is by systematically exploring what it is not. In a loose way, I will apply that method here. I will try to make clear what I believe ethnosemantics to be by talking for a bit about what it is not. This strategy particularly recommends itself because in recent years there has appeared a fair amount of literature and a large amount of conversation which indicates that some anthropologists, who do not themselves do ethnosemantics, harbor some specific misconceptions about the nature of that field. * I should hasten to add that the critics of ethnoscience do not bear the

* Most of these misconceptions, although not all, may be found in Harris (1968:568-604) and Berreman (1966).

full blame for their misunderstandings of the field. Not infrequently the misunderstandings revealed in criticisms of ethnoscience are attributable less to obtuseness on the part of the critics than to opaqueness in the writings being criticized. Some of the more important work in ethnosemantics has been presented in a confusing and/or confused style, and the fact of its being misunderstood must be considered at least as much the authors' as the critics' fault. A logician has said in speaking of the Principia Mathematica^a, that when Bertrand Russell does logic he is always right and when he talks about logic he is almost always wrong. Ethnoscience, like logicians, are not infallible when they interpret the significance of their own technical accomplishments.

I would also like to call attention at the outset to an obvious fact: namely that it is just as easy to do ethnosemantics badly as to do anything else badly. (Upon occasion I have personally found it distressingly easy.) Inaccurate or trivial work can be done in ethnosemantics as in any other field. When, in attempting to set straight some erroneous criticisms that have been made against ethnosemantics, I speak of what ethnosemantics is, I mean what it is when it's done right.

On the other hand, none of us wishes to evaluate an entire field in terms of its worst productions. A criticism of a field based on an artful selection of its errors and trivia is a criticism itself devoid of much accuracy or importance. In the growth of a science, it is the peaks of achievement that count, not the ~~troughs~~^{troughs} of error. In the evaluation of progress, let us look not at the twisted trail of false starts and needless detours, but at where in fact we have got to.

There are at least six important misconceptions about ethnographic semantics extant in the literature. It is perhaps worthy of passing note that several pairs of these misconceptions are mutually contradictory, for example that ethnographic semantics is anti-empirical on the one hand, yet anti-theoretical and exclusively concerned with trivial methods for data shuffling on the other. When we have considered how and why ethnoscience

is not each of these things, I think we will have a fair idea of what it is.

1. Ethnosemantics does not take native generalizations at face value.

Some anthropologists have criticized the cognitive approach to ethnography in the mistaken belief that (1) ethnoscientists believe everything their informants tell them and (2) ethnoscientists believe only what their informants tell them. According to this view, the cognitively oriented ethnographer presents as his model of the culture or society just a summary of the explicit ideological statements of the local establishment. As Marvin Harris (1968:590) has suggested, this approach would produce an ethnography of the contemporary United States in which race is irrelevant to economic opportunity, adultery never occurs, and the major purpose of the meetings of learned societies is intellectual intercourse.

There are at least two errors here, one obvious and one subtle. In the first place there is the tacit assumption that an ethnographer who pays careful attention to the local ideology is ipso facto a maniacal super-Platonist who can't tell the difference between ideology and practice. Perhaps there does exist some confusion in British social anthropology on this point, for example in the interminable debate on "jural rules" and related matters. But the position of the new ethnography is straight-forward. Cognition is cognition and behavior is behavior. If the native's cognitive world is populated by seventy-two different kinds of yams and thirty-six different kinds of ghosts, then that's that. I know of no case where an ethnosemanticist has confused a native's beliefs about ghosts with actual ghosts.

A somewhat subtler error is the notion that the ethnoscientist equates the explicit ideology with the culture, i. e., with the native's system of belief, his cognition. According to this slightly more refined view, the semantic ethnographer of the United States would describe it as a place in which adultery and racial discrimination indeed flourish but in which the natives are unaware of these facts. The accusation is that ethnoscientists

tend to give excessive credence to natives' broad generalizations, producing descriptions of cultures that contain a spurious homogeneity, simplicity, and harmony--ethnographies, to use Harris's felicitous phrase, "in which the rules of behavior dominate the rules for breaking rules."

The fact that Harris and similar critics ignore is that very notion of "rules for breaking rules" has come from the new ethnography. The confusion is apparent when Harris (1968:590-591) attacks Goodenough (1965) on this issue for his treatment of the now famous incident in which a Trukese father strikes his married daughter, "thus violating five of the six rules in Goodenough's emic 'Duty Scale.'" Harris remarks scornfully that "Goodenough takes comfort from the fact that the woman in question had in turn been breaking another series of rules!" and he concludes, "So blatant a failure of a set of rules to predict behavior requires a rewriting of the rules." But, what Goodenough has in fact presented is an instance of actual behavior in conformity with a rule for breaking the rules. Goodenough's argument is that Trukese culture prohibits fathers from hitting their married daughters except when the daughter has violated certain cultural rules to an extent which is unconscionable from the Trukese point of view. Goodenough has uncovered a Trukese cultural rule which is formally analogous to the American cultural rule prohibiting homicide except in self defense. When Goodenough characterizes the father's blow as "poetic justice" and "just what she deserved," he is not expressing his personal moral judgment, as Harris appears to think; he is underlining the fact that Trukese cultural rules provide for fathers striking their married daughters just on one set of narrowly defined occasions, and that this is one of those occasions.

The moral of Goodenough's story is precisely that cultures, seen as cognitive systems, are extremely complex, and that cultural rules characteristically contain except and unless clauses. Goodenough's early paper on residence rules in Nakanai (1956) makes the same point. He lists

half a dozen or so variables which are evaluated by an individual in deciding where to reside postmaritally. The actual decision procedure is represented by a set of contingency statements to which are inputted the evaluations on the relevant variables. The set of contingency statements forms a rather complex network, formally analogous to a switching circuit diagram or the flow diagram of a computer algorithm. The central idea is that global characterizations such as matrilocality or patrilocality do not cut fire enough to match the complexity of the native cognitive system.

It is the appreciation of the detail and complexity in human cognitive systems that has been one of the major contributions of ethnosience to cultural anthropology. The realization of just how complex even a small area of culture can be has led ethnosemanticists frequently to restrict their study to individual lexical and cognitive domains, which has in turn led to the charge that ethnosience studies trivialities.

2. Ethnosemantics is not concerned with methodology for its own sake.

The basic working assumptions of ethnosience are (that) (1) human behavior cannot be understood without taking into account human cognition and (2) that human cognition is complex. These beliefs lead naturally to the methodological hypothesis that traditional anthropological techniques of recording and analyzing native systems of belief are insufficiently rigorous and detailed to capture the fine complexity of cultural systems. We really know very little about how people organize their cultural knowledge and we need new techniques for finding out. It is this attitude that has led to the series of methodological studies associated with the names of Goodenough, Lounsbury, Metzger, Williams, Conklin, Frake, Romney, D'Andrade, and others. I am not at the moment defending the substantive assumptions which lead to the conviction that anthropology needs some new methods (although I certainly agree with these assumptions), I am just arguing that the methodological concerns of ethnographic semantics are not fortuitous

but are derived from a substantive theoretical position. The major outlines of that position can, I think, be delineated as follows:

- (1) Culture is essentially a matter of shared cognition.
- (2) Human cognition is not in principle unamenable to rational inquiry.
- (3) Among the relatively concrete and understandable phenomena at our disposal, language and speech appear to most directly express what we suspect may be the organizing principles of human thought.

If one accepts the above three assumptions, then I think it becomes clear why ethnosemanticists have been vitally concerned with the development of new research strategies and have looked especially toward linguistics and cognitive psychology for candidates. The critic may still legitimately object "I do not believe in the importance of cognition and language and therefore I have no use for research methods which are founded on this belief." But it is not a valid criticism of ethnosemantics to characterize it as concerned with rigorous method for its own sake.

3. Ethnoscience is not anti-theoretical or anti-comparative.

As suggested above, much of the impetus to the formation of the new ethnography was a growing dissatisfaction with traditional ethnological categories such as matriliny, patrilocality, unilineal descent, and so on. This imposition of an a priori classificational scheme on richly complex cultural materials is precisely what Leach refers to in his article on "butterfly collecting." (1961:1-27)* Ethnoscience shares with a certain

* I must confess that although I agree totally with Leach's statement of the problem in that article, I am hard-pressed to make much out of his proposed solution.

segment of British social anthropology the conviction that inter-cultural

comparisons based on such gross classificational schemes are likely to be no more meaningful than comparisons of nominal systems in grammar in terms of the Latin categories nominative, genitive, dative, accusative, and ablative. In short, one has to isolate comparable units before one can engage in reasonable comparison. Hence, the emphasis in ethnosience on emics, so-called, the analysis of a cultural system or subsystem in its own terms as a precondition to the comparison of different systems.

But to say that one should compare only things that are truly comparable is not to say that one should not compare. The very provenience of the emic/etic distinction, namely phonology, should make it clear that the guiding spirit of an emic approach is to rid oneself of his preconceptions about universal structures so that the data may be analyzed objectively to reveal the true universal structures. Phonology is in fact an excellent case in point. Far from becoming (or remaining) a particularizing discipline, phonology has enjoyed a lively debate about universal laws of sound structure ever since the discovery of the phonemic principle. The phonemic principle was in fact a very strong empirical hypothesis about the universal structure of sound systems. This hypothesis led to the intensive investigation of sound systems of many languages in their own terms and to the continual comparison of the results of these analyses in the search for stronger universal hypotheses of sound structure. Phonology today is a field rich in general theory including Greenberg's implicational universals and the Jacobsonian and other distinctive feature systems and their formal incorporation into the theoretical framework of generative phonology. The emic approach in ethnography as in phonology is fundamentally the very opposite of a particularizing, anti-theoretical position. Rather it incorporates a firm belief in the existence of universal structural principles in the phenomenon under study and a commitment to the diligent search for those universal, pan-human structures, unimpeded by preconceptions as to the outcome.

Ethnographic semantics, although not nearly so well developed a field as phonology, is already beginning to produce just such universal

hypotheses of human cognitive structure. Lounsbury's theory of kinship extensions is one example. The axiomatization of human cognitive structures involved in taxonomic systems growing out of the work of Conklin, Berlin, and myself is another. Greenberg's work on semantic universals, especially semantic marking, is another case in point. Berlin's findings on formal and substantive semantic universals in the growth of ethnobiological vocabulary are additional examples, as are some of his findings on universals in the lexical classification of shapes and textures. Geoghegan's axiomatic theory of cultural systems for the processing of classificatory information is yet another example. Additional examples could be cited and I apologize in advance for the inevitable slights. I also apologize for being unable to present a detailed discussion of these findings and theories, but even an adequate summary of any one would require almost the full time allotted to me here. However, Berlin has discussed some of these findings already.

4. Ethnographic semantics is not limited to domains in which there are pre-existing etic grids; nor are its methods inapplicable to domains of social organization not involving genealogical relationships.

Some of the successes--or excesses depending on your point of view--of the semantic analysis of kinship systems have led to the mistaken conclusion that objective semantic analyses can be performed only in a small class of semantic domains, namely those domains for which we possess a priori a classification that is so fine it necessarily incorporates all the distinctions made in any native system. Such an a priori classification is sometimes called an etic grid. The traditional notation of biological kin-types, so called, and its various recent modifications and improvements embody one such classificational device. The international phonetic alphabet and the Jakobsonian distinctive feature framework are linguistic prototypes.

The belief that semantic analysis can only be performed when an etic grid is available seems to be connected with an odd misconception about the history of phonology that appears to be wide-spread in anthropology. *

* I must confess that I do not recall having seen this stated explicitly in print, but it seems implicit in much anthropological discourse on these matters both written and spoken.

The idea seems to be that the development of an adequate universal phonetic grid temporally preceded the successful emic description of individual phonological systems; only after linguists were able to figure out all the possible sounds and sound contrasts that people could make were they able to perform phonemic analyses. The historical facts are quite to the contrary. Linguists first achieved a large number of emic analyses of the sound systems of various languages and, as evidence from a wide number of emic studies accumulated, the general outlines of the universal phonetic possibilities began to emerge. In fact, there is probably much less agreement right now within linguistics on the general outlines of universal phonetics than many anthropologists imagine.

In any case, presence of an etic grid is not a precondition to successful semantic analysis. It can often be a help but can never be taken for granted. Berlin's experience in Tzeltal ethnobotany has shown that using Western botanical classification as an etic grid handles most of the data, but not all. Tzeltal botanical classification, with respect both to plant classes and the features defining and distinguishing the classes, usually "makes sense," so to speak, in Western botanical terms. But there are occasions where Tzeltal cognition of the botanical realm does not make sense in Western terms, as when, for example, two taxa are distinguished on the basis of what is in Western terms a botanically irrelevant attribute such as size or color of fruit.

Closely related to the idea that ethnographic semantic analysis always requires an etic grid, and intimately involved in the accusation that ethnosemantics is "the science of trivia" (1968:591),^{Harris} is the notion that semantic analysis is inapplicable in any cognitive domain of social importance. Lounsbury's work on Trobriand kinship and on Crow-Omaha systems in general is a sufficient counter-example to this claim, although this work does employ the genealogical etic grid. However, much of Keesing's excellent work on the Kwaio employs semantic analysis not based on an etic grid, as do several significant studies of residence including Goodenough's on Nakanai and Geoghegan's (1968) on Samal. (Again, apologies to colleagues I may have slighted.)

A particularly interesting example of an ethnosemantic analysis not employing an etic grid and of unquestionable sociological significance is that performed by Irving Zaretsky on the religious argot of San Francisco Spiritualist Churches (Zaretsky 1969). Zaretsky found that many common English words were being used in ways that were clearly not the normal meanings of those words. An ethnosemantic analysis of all such words revealed a subset with the following interesting properties. Words belonging to this set have unmistakable although vague connotations of supernatural "spirit forces" at work in the world, and in particular through the agency of the medium. However, the referential meanings of these forms are extremely nebulous. This is not a question of the investigator's being unable to discover the referential meanings but rather that Zaretsky has shown through detailed comparison of the contexts in which these expressions are employed that the referential meanings are in fact very vague. He then goes on to show two important ways in which the very referential vagueness of these words contributes to the perpetuation of the social organization in which they are employed. First, referential vagueness permits the medium to transmit acceptable messages from the spirits to a parishioner who has written down a specific question that the medium has not seen. For example,

- Ethnographer: Can you recall any message you received in church recently?
- Informant: Well let's see... She told me I was going to get drafted...
- Ethnographer: Did she actually come right out and tell you you will get drafted, just like that?
- Informant: Well, not exactly. She kept muttering about how I have upset conditions around me and how she saw the color green in my vibration... She just went on like that for a while... Well, I know what she was talking about because I waited to hear from my draft board all this time... But I certainly didn't want her to tell me that I will be drafted...
- Ethnographer: How did you know what she meant by condition, vibration, environment?
- Informant: Well I didn't... No, actually it was obvious... I guess she could tell psychically what bothered me... Well, I don't know, but I think it was pretty obvious what she meant... (Zaretsky, 1969:124)

This referential vagueness of the terms employed permits the ardent parishioner to endow them with whatever particular referential interpretation he wishes in each occasion of use. Secondly, this same referential vagueness protects the medium from the possible legal charge that she is engaged in the illicit activity of forecasting specific future events for money.

5. Ethnosemantics, despite its emphasis on research techniques that take account of local variations in emic structures, is not anti-evolutionary, anti-historical, or anti-etic.

I will dwell only briefly on this point, as Berlin's talk has treated some of the relevant data in considerable detail. It is certainly true that most empirical work in ethnographic semantics has until recently been devoted to synchronic emic studies. This fact has convinced some people that the field is inherently anti-historical and committed to an unyielding form of the doctrine of extreme cultural relativity. One cannot help but be reminded of the accusation frequently leveled against generative grammar

until about five years ago that it was anti-historical or at least incapable of dealing with the data of language change. (I think it was at about that time that a noted Neo-Bloomfieldian is reported to have remarked, "My God, I hear they're going to invade historical linguistics next!") In the last five years a large number of historical studies by generative linguists have appeared, the most recent of which is a comprehensive theoretical treatise on historical linguistics from the generative point of view (King, 1969).

Ethnosemantics has never been anti-etic. In fact, as I have argued above, the guiding spirit of the emic methodology has always been to take objective account of the local emic organization of cultural materials precisely in order to discover both the universal formal structures that dictate the shape of all emic cultural systems and to crack through the local emic codes to the etic semantic universals that underly them. There has never been any question in ethnosemantics about belief in the psychic unity of mankind. This has always been taken for granted. The discovery of as much as possible about the nature of the human psyche has always been the ultimate goal. The concern with emic methods has been dictated by the obvious fact that our common human psyche chooses, at least on the surface, to organize itself very differently in different cultural systems.

Linguistics is beginning now, only after years of careful analysis of individual emic systems, always with an eye to possible universal generalizations, to achieve some fairly clear and powerful general theories of the universal structure and sound content of language. But it is unlikely that the important results of modern linguistic theory with respect to universals of language would have been achieved if that discipline had refused to take account of the irrefutable and unavoidable facts of the emic diversity of languages. *

* Notwithstanding some of Chomsky's more extreme statements in Cartesian Linguistics (1966), from which some have inferred his position to be that generative grammar has no historical roots in linguistics of the period between, say, 1700 and 1950.

A concrete example of the use of emic method being confused with the notion that the user is committed to an ultimate emic nominalism with respect to semantic and cognitive systems is illustrated by recent work on color terminologies. Conklin (1955) showed convincingly that Hanunóo color terminology in fact encodes a lot of non-colorimetric information. In fact, there is some question, as Conklin points out, whether from the point of view of Hanunóo culture it makes any sense to talk about "color" terminology at all. What Conklin demonstrated is that there is an emic structure to cognitive domains that must be taken account of methodologically if one is to arrive at anything like a meaningful cross-cultural generalization. In commencing our search for semantic universals in color terminology, Berlin and I never doubted for a moment the validity of Conklin's findings, and our results in no way--I repeat, in no way--contradict his. We knew we had to employ a method that would first uncover the local emic organization of the domain of color and then find some way to get behind or beyond that to see if there were in fact etic universals at another level of structure. I think it is no accident that these etic universals were stumbled upon by people trained in ethnoscientific method and who were firmly committed to a procedure that takes account of the inconvenient but unalterable fact of emic cultural diversity.⁶ Additionally it turned out that our findings had historical and evolutionary implications. I will not recapitulate the details here. I wish only to point out that this constitutes a counter example to the claim that ethnographic semantics is necessarily anti-historical.

6. Ethnoscience is neither anti-statistical nor unconcerned with prediction.

I have insisted on the distinction between ethnosemantics on the one hand and an approach to ethnography that includes an ethnosemantic component on the other. Some critics of the new ethnography have been unaware of this distinction and have consequently criticized ethnosemantics for not being all of ethnography. The most usual form this criticism takes is the accusation that the new ethnography so-called is either unwilling or

unable (or both) to deal with the statistical facts of the distributions of persons and resources in a society, that is, with social organization.

It is true that a cardinal tenet of cognitively oriented ethnography is that one must take careful account of the cognitive devices that constitute the culture bearer's decision making process if one is to arrive at a successful description of the outcomes of these decisions. Resources, including human resources, do not allocate themselves to various slots in the social economy but rather resource allocation is achieved as the result of myriad individual decisions made in accordance with the cognitive structure we call the local culture (or sub-cultures). There are at least two ways of checking empirically that one's construction of the native actors' decision making process is correct. First one can assess native actors' reactions to actions on the part of their fellows. The cognitive model predicts which actions will be judged appropriate under which circumstances. We have already considered an example of this type; Trukese find a man's striking his married daughter inappropriate, if not down-right insane, except under certain specified conditions.

However, this is not the only kind of test one can make of a cognitive model. The stronger test requires that one predict not only the native's judgments of appropriateness after the behavioral facts but that one predict actual behaviors before the fact. To do so one obviously has to have information outside of the cognitive model, namely the inputs to that model. For example, suppose a cultural rule states in part "reside post-maritally with the husband's matri-sib if that sib (a) is localized (i. e., is in possession of territory) and (b) owns uncultivated land." If we wish to predict where a given individual X will reside after marriage, we will have to know, in addition to the fragment of a cultural rule just given, the sex of X, which matri-sib X (or X's husband) belongs to, whether that matri-sib is localized, if so where, and whether or not that matri-sib controls unused land, among other things.

To predict distributions of actual residence patterns on an aggregate scale, we have to furnish as input to the postulated cognitive model the joint distributions of matri-sib membership, matri-sib localization, wealth in land, and so on, for the entire population. This approach has been successfully employed to predict several sorts of statistical distributions of economic and demographic data reflecting the outcomes of individual applications of cultural rules. For example, the varying frequencies with which Society Islanders in diverse communities adopt members of different kin and non-kin social categories have been successfully predicted by a model that posits a uniform set of cultural rules into which are plugged the appropriate local demographic and economic parameters in each community (Kay 1963, Hooper and Kay 1965, Hooper 1969).

A more recent and complete example is Geoghegan's (1969) study of "Decision-Making and Residence on Tagtabon Island" in the southern Philippines.

On the basis of controlled elicitation and semantic analysis, Geoghegan posits a cognitive model of how an individual Samal decides how to reside. The model takes into account various emically defined attributes of the age, sex, social status, economic status, kin relationship, and so on, of the person making the decision and of various other persons related to him. The outputs of the model are diverse modes and locales of residence; for example, "I shall live in my own house, and locate it near my father's house," or "I shall live alternately in the house of my wife's mother and the house of my own father." The model applies not only to residence choice at marriage, which may be a more or less conscious affair, but also to the tacit decision which may be attributed to every individual by virtue of the fact that he is residing somewhere. This model, constructed exclusively from data obtained through systematic elicitation from selected informants, nevertheless applies to the entire population in that it takes into account the relevant social identities of the native user. We may note in passing that the model itself is a rather intricate mathematical object,

which Geoghegan derived directly from his analysis of natives' statements;
~~The model itself~~ is certainly not consciously articulated by any one informant or group of informants.

The model is tested as follows. Detailed census data give the actual distributions of residence modes and locales in the population. The census data also give distributions of age, sex, social statuses of various types, and so on. From the latter type of data, Geoghegan estimates the probability that an arbitrary individual will answer yes to each of the many questions implied by the cognitive decision model (for example, Yes, I am adult; yes, I am married; yes my spouse is living, etc.) To simplify greatly, let us suppose that the cognitive model specifies a particular mode of residence (call it A) for an adult male whose spouse is living. Estimating from the census data the individual probabilities that an arbitrary person is male, adult, and possessed of a living spouse, Geoghegan multiplies these three numbers together to get an estimate of the probability that an arbitrary person will chose residence of type A. (Actually the individual conditional probabilities are employed.) Geoghegan then multiplies the probability of each type of residence decision by the number of persons in the total population. The result is the hypothetical distribution of residence types in the population that is predicted by the cultural rule. This predicted distribution is then compared to the actual distribution of residence types taken directly from census data. The two distributions are ^{virtually} identical. The largest proportional error in prediction ^(11%) involves a predicted frequency of eight cases against an observed frequency of nine cases. The ^{average} proportional error of prediction is about 6%.

I know of no case in anthropology where this fine a prediction of comparably significant behavioral data has been achieved. Not only is ethnosience not opposed in principle to prediction, but I think it is fair to say that it has achieved substantial practical results in that direction.

Conclusion

Having discussed ethnosemantics by contrasting it with a number of things which it is not, let me summarize in a more positive vein. Ethnographic semantics is the study of the meanings of the expressions of a language, the system formed by the interrelations of these meanings, and the cognitive system which lies behind the system of meanings and which is realized in large part through them. As such, ethnosemantics is part of anthropology as well as part of linguistics and psychology. It is part of anthropology in that the culture shared by a group of people consists essentially in the cognitive system that makes the actions of one intelligible to another and makes possible all truly social intercourse, including competition, regulated conflict, and insult, as well as gentler modes of transaction.

Ethnosemantics is employed in ethnography as a means of arriving at the cognitive system, or systems, employed by people as a device for classifying their environment, evaluating various states of that environment, predicting what the outcomes of the various behavioral possibilities open to them will have on that environment, and ultimately selecting a course of action. As we have seen, cognitive models alone do not predict overt behavior. But when the cognitive model is supplied with the information it specifies as necessary for reaching a decision, it can predict overt behavior accurately.

There is also a point at which the disciplines of anthropology, linguistics and psychology coalesce, the study of the human mind. I am sorry if that word offends anyone, but to pretend that human beings do not have minds because a mind has never been seen or touched would require that we reject virtually all highly abstract and therefore useful scientific concepts, including, for example, gravity, relativity, and probability. The theoretical implications of ethnosemantics to the study of mind are twofold. First we are beginning to get a glimpse of the formal structure of semantic and cognitive domains, and even of their articulation into larger sub-systems of the total cognitive system. I refer here specifically to the theoretical

work of Goodenough (e. g. , 1965), Geoghegan (1968) and its empirical applications by Goodenough, Geoghegan, Keesing and others in representing formally not only individual cognitive domains but their mutual integration into larger cognitive systems.

Secondly, in the emergence of substantive and formal semantic universals, we are perhaps seeing the first glimpses of an explicit and empirically motivated description of that "psychic unity of mankind" in which anthropologists have so long believed. Lounsbury's work strongly suggests that ^{we} may soon attain a fixed finite vocabulary of universally recognized basic kin relations which are variously elaborated into the widely differing emically defined kinship systems of the world by a set of orderly and precisely describable processes. It is hard to resist an analogy between this and the recent findings on color terminology. Once again we see a universal set of fixed focal points underlying the apparent randomness of the distribution of color concept boundaries in various cultures. The recurrent themes are that emic diversity and relativity are underlain by certain universal etic semantic constants and that the ways in which each emic system defines its own genius out of the universal vocabulary of basic concepts are orderly and subject to rational discovery.

BIBLIOGRAPHY

Berlin, Brent, Dennis E. Breedlove, and Peter H. Raven

- 1968 Covert Categories and Folk Taxonomies. American Anthropologist 70, 2:290-299.

Berreman, Gerald D.

- 1966 Anemic and Emitic Analyses in Social Anthropology. American Anthropologist 68:346-354.

Black, Mary B.

- 1969 Eliciting Folk Taxonomy in Ojibwa. In Cognitive Anthropology. Stephen A. Tyler, ed. New York, Holt, Rinehart, and Winston.

Chomsky, Noam

- 1966 Cartesian Linguistics. New York, Harper and Row.

Conklin, Harold C.

- 1955 Hanunoo Color Categories. Southwestern Journal of Anthropology 11:339-344.

Geoghegan, William

- 1968 Information Processing Systems in Culture. Working Paper No. 6, Language-Behavior Research Laboratory, University of California Berkeley. To appear in Explorations in Mathematical Anthropology. Paul Kay, ed. Cambridge, M. I. T. Press. Forthcoming.
- 1969 Decision-Making and Residence on Tagtabon Island. Working Paper No. 17. Language-Behavior Research Laboratory. University of California, Berkeley.

Goodenough, Ward H.

- 1956 Residence Rules. Southwestern Journal of Anthropology 12, 1:22-37.
- 1965 Rethinking "Status" and "Role". In The Relevance of Models in Social Anthropology. New York, Praeger.

Harris, Marvin

- 1968 The Rise of Anthropological Theory. Crowell, New York.

Hooper, A. and Paul Kay

- 1965 Tahitian Fosterage, Cultural Rules, and "Social Structure." Paper read at the annual meetings of the American Anthropological Association.

Hooper, Anthony B.

- 1969 Adoption in the Society Islands. In Adoption in Eastern Oceania. V. Carroll, ed. (ASAE Monograph No. 1) Honolulu, University of Hawaii Press.

Kay, Paul

- 1963 Tahitian Fosterage and the Form of Ethnographic Models. American Anthropologist 65, 5:1027-1044.

King, Robert P.

- 1969 Historical Linguistics and Generative Grammar. Englewood-Cliffs, Prentice-Hall, Inc.

Leach, Edmund R.

- 1961 Rethinking Anthropology. London, Athlone Press.

Quine, W. V. O

- 1960 Word and Object. Cambridge, M. I. T. Press.

Zaretsky, Irving I.

- 1969 The Message is the Medium: An Ethnosematic Study of the Language of Spiritualist Churches. Ph. D. dissertation. University of California, Berkeley.